THE CONCEPTION OF CANCER BEFORE AND AFTER JOHANNES MÜLLER*

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IT IS becoming increasingly the vogue to commemorate centennials. There is frequently little true sentiment in these gestures; the centennial becomes an excuse for a celebration or for an exploitation. Too often the attitude is not that of reverence for great events of the past; instead, the past becomes only a convenient background against which the presumed progress of the present may be displayed by contrast. I mention this attitude because I venture to bring to you tonight the materials for a medical centennial, and I do not want my object or my attitude mistaken. Neither is in the vogue. I want, if I can, to revive the past and make it live just for a moment in some of the realities it possessed. And my attitude is that of an almost religious obeisance to one of the great men and one of the great events of medicine. Tonight I bring you nothing new but only the hope that in reviewing together here briefly the life of a man we may pause together in our busy ways of the present to revere his memory and to acknowledge a debt.

There have been men—and you can count their number on the fingers of your hands—who have taken up great boulders far beyond the strength of ordinary men and with them have laid down the foundation upon which medicine is erected. Johannes Müller was such a man. The reason that we might celebrate in his honor this particular year of 1938, and the reason for my paper tonight will become evident from a quotation that I read from his works. His words and his sentences as you will see form the foundation for the modern conception of the nature of cancerous growths. In quoting him I pick and choose only a few pertinent sentences from a dozen pages of his book entitled: "On the Nature and Structural Characteristics of Cancer and of those Morbid Growths Which may be Confounded with it." The book was published in 1838.

The first sentence I quote shows with pathetic clarity the lack then

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of any adequate means of differentiating malignant from nonmalignant growths. Müller writes: "Usually it is regarded as an infallible sign of malignancy if a tumor, after having been extirpated once or twice, returns to the same spot."

As to the need of some better criterion he says: "All these circumstances, while they render it extremely easy to confound tumors naturally innocent and dangerous only under certain circumstances, with such as are by nature malignant, afford many additional reasons for seeking some surer means of distinguishing between the two than we at present possess."

He then goes on to say that the classification of morbid growths is wholly deficient; that there is literally none. As a first step toward clarifying the confusion he has made a collection of tumors at the Royal Museum of Berlin. Of this collection he says:

"On close inspection of the preparations, many were met with presenting such peculiarities that it was not possible to assign them any certain place in accordance with the state of knowledge at that time. Soon it became evident that if the classification of so many important objects were to have any real value, it would be necessary to devote many years to the examination of them and of other fresh pathological specimens; and to this task the author accordingly betook himself."

Müller next turned to his microscope to utilize the method that he was instrumental in introducing into pathology; he examined under the microscope the tissues in his collection.

He says: "As early as the year 1836 the author had recognized with the microscope the cellular nature of various morbid growths . . . the cells unless magnified from 400 to 500 times generally look like granules; but on the employment of a high power the cellular structure of most morbid growths becomes apparent."

He goes on further to say that the cellular form in a neoplasm resembles in general features that of the tissue in which the growth occurs. And then, and perhaps most important of all, he relates the cellular physiology of neoplasms to that universal for normal tissues. He says:

"The part which cells sustain in the composition of all morbid growths has recently acquired additional importance from the investigations of Schleiden and Schwann. The researches of the former relate to the development of the young cells of plants from nuclei formed in the interior of the parent cells; those of the latter refer to the analogy

between animal and vegetable structures. According to Schwann all tissues in the embryo are formed from cells, which are themselves developed from nuclei; the growth being the result of fresh formations of cells, which afterwards undergo transformation into other tissues. These observations . . ." (and I break my quotation here to mention, what I shall deal with more fully later, that Schwann was the pupil of Müller and made his investigations on the cell in Müller's laboratory and under Müller's inspiration). I continue: "These observations led the author to examine morbid growths very carefully. By employing a high magnifying power, cells were observed in malignant growths. . . . The nuclei of the cells were discovered . . . in many instances too, young cells were found. Thus, then, as might have been anticipated, did examination of morbid structures confirm Schwann's observations touching the development and growth of healthy tissues."

The statements which Johannes Müller makes are the beginning of the cellular pathology which his brilliant young pupil Virchow expanded during the next ten years into a basic concept. Johannes Müller in 1838 in unmistakable language—and for the first time—said that cancer is cellular and that the cellular form resembles that of the tissues from which the cancerous growth springs. There in his discovery is the foundation for all modern classification, diagnosis, therapy and research in oncology.

Tonight, using the discovery of Johannes Müller as the dividing point between ancient and modern, I want to outline briefly the conceptions of cancer before and after his work. But mainly I wish to recall to you the man whose discovery we are privileged to celebrate this year.

The external manifestation of neoplasms had, of course, been recognized from early antiquity. The Ebers papyrus gives evidence that the Egyptians knew of tumors, the mention is of a lipoma. Hippocrates wrote of cancers, and used the term carcinas for inflammatory swelling and the term carcinoma for a neoplasm. Celsus went even further; he recognized clearly visceral forms of cancer and attempted to differentiate clinically malignant from benign neoplasms. Galen observed cancers with considerable accuracy but at once obscured good clinical description with theory; cancers came under his doctrine of the four humors. Cancer was due to the concentration of black bile. This humoral theory persisted until after the discovery of the circulation of the blood and of the lymphatics when Malpighi, Louis and Astruc advanced the idea that

cancers were clotted and degenerated lymph. The dominating view to reach the nineteenth century was that cancer was essentially a general disease; that the growth itself was merely a local manifestation analogous in this respect to the pustule in smallpox. Such was the prevailing view until 1838 when Johannes Müller demonstrated that the cancer consisted of an abnormal growth of abnormal cells.

It is true that before Müller, Bichat had made an important step; he had directed the views of pathologists from organs to tissue or, as he called them, "membranes." Morgagni has said that some organ was always the seat of a disease. The fact that identical symptoms might develop when different organs were affected caused some discord in the harmony of this simple view. It was suggested by Pinel—better known for his work in psychiatry than in pathology—that different organs might have similarities in structure and disease in organs with anatomical similarity though in different parts of the body, might account for this confusion. Bichat took up this idea and carried it further with the conception that organs were built up of basic membranes, tissues which were variously distributed in the organs. Bichat made extensive tests in differentiating the tissues even to tasting them but he did not make microscopic studies. He succeeded in removing the seat of disease from the organ as a whole to its tissues. Pathology became at his hands membranous pathology. It was Müller who first made pathology cellular; and the disease he dealt with was cancer.

And now in approaching the man and his work, I ask you to look back with me for a moment at the situation of his native land, Germany, at the opening of the nineteenth century. Müller was born in 1801. The Germany that he faced was socially and economically the Germany of a little more than a century later—the Germany of 1918. It was post-war Germany; a country in the depths of a depression. It was a hungry, sorry, bitter country, bruised and battered. Like a man shocked by too fierce contact with reality it was prepared to withdraw from reality—to turn to philosophy for escape.

The situation as you will recall developed thus: Near the close of the eighteenth century the last political act of Frederick the Great was the establishment of the League of Princes which brought back a semblance of unity to the Old Empire. In France the trends were in the opposite direction; the Revolution was under way. At first the philosophy of reason interested the educated Germans; but the shift to bloodshed and

the overthrow of the government frightened them and alienated them from the cause of liberty. The French emigrés fled to Germany and took up arms there. France protested; Germany soothed and quieted; the French demanded the abolition of the feudal rights of the German Princes in Alsace; again Germany soothed and quieted. Even the fact that Marie Antoinette was the sister of Emperor Leopold did not bring the Germans into war. At most they concluded a defensive alliance with Prussia, and sent a note of protest to France. The French countered with a declaration of war. That was in 1792; the war ended in 1814. It included the rise of Napoleon and his defeat and domination of the German people. It concluded with the battle of Leipzig and the taking of Paris. The war was over—twenty-two years of war and subjugation. Germany was finally victorious but she was left exhausted facing reconstruction.

There was the war generation, and the dead; there was poverty, debt, actual want. In such situations as these there are reactions; and these reactions are wholly unpredictable. In Germany the reaction came as a wave of idealism; a turn from practical, factual views to romanticism, even mysticism. Why centuries before a similar emotional reaction had taken the form of the dancing mania, why in the twentieth century it takes the form we see today; and why in the nineteenth century it was toward romanticism and the worship of beauty—no one can say; the psychology of such national movements is beyond our knowledge for explanation.

This romantic movement was not an affair alone of poets and imaginative writers, of a cult of beauty that revived interest in the medieval architecture and the learning of the East. It was entered into by physicians and naturalists, men whom, in a more rational environment, we should call scientists. The movement was toward what was called in Germany natural philosophy, a quite different use of the term than that applied in England where natural philosophy was natural science. In medicine the movement took the form of speculation, philosophical considerations and especially the attempts to develop systems that in their completeness would give knowledge in totality.

Now we in medicine today have been educated away from this type of thought. Our emphasis today is continually for novelty—for the discovery of the new as discrete bits of knowledge. I suspect that we err in this direction as far as the romantic natural philosophers of 115 years

ago erred in the opposite direction. The weaving of facts into a fabric of order and system is as important as finding the facts but synthesis must come after the facts; so far all extensive attempts have been abortive. We have reacted against this philosophical approach and for good reasons. In the past it has almost invariably led to absurd and untenable systems and to utter disregard of the practical. We have not yet attained enough of the material for synthesis; in the Germany of a century ago it was even more lacking. The fine theories in medicine that aroused the ecstasy of the founders and followers of these fads were not developed in the clinic, the laboratory, or at the bedside. They were spun in armchairs from material as tenuous as the fabric of cobwebs but spun with a pathetically misdirected but feverish enthusiasm. It was an unhealthy state of science—a delirium.

Every fad of medicine, every discovery of science, was made into a system; each system had staunch adherents. Paracelsus was revived and walked again in his followers; the Brunonian system was revered by its disciples; and the philosophy of Schelling permeated everywhere. Mind and matter were identical. Hence the laws of Nature must be capable of direct demonstration in unconsciousness and consciousness in its turn must manifest itself as the laws of Nature. Consequently the laws of nature could be discovered speculatively. This intellectually exalted introspection is, in its subsequent degenerated form, familiar in the works of Mary Baker Eddy and similar writers. But in those days it led in medicine to the development of what Karl Hoffman called his "ideal pathology." Disease was, according to this pathology, a retrogression to a lower grade of evolution. Rickets signified an evolutionary reversion toward the mollusk. Digestive disorders with vomiting had their analogy in the cud chewing of the cow and therefore indicated a regression toward the cattle stage. The one system of healing to persist for any length of time from this phase of German thought was that founded in 1808 by Samuel Hahnemann.

Virtually no sound scientific achievement came from Germany during the first two decades of the nineteenth century. And yet by the fifth decade Germany led the world in medical science—had literally laid the foundation for the type of medicine in which we here were all trained. The man responsible for this change was Johannes Müller. The turning point from natural philosophy (again in the German sense of the word) to natural science came in 1833 when Müller published his Manual of

Human Physiology. It was around him that the great school of Berlin was built—the school that so strongly influenced medical education and practice in America in the late nineteenth and early twentieth centuries. Glance with me at the names of some of the leaders in the medical revival that gave Germany its preeminence: Schwann, Henle, Kollicker, Du Bois-Reymond, Helmholtz, Virchow. Each one of them was a pupil of Müller, trained in his methods, trained to carry on and expand the science he taught. The only great figures of the period who did not spring from his hands were the two clinicians, Schönlein and Wunderlich. It was Johannes Müller who aroused in Germany the enthusiasm for scientific investigation in the basic medical sciences.

Müller was born in Coblenz on July 14, 1801. At that time the French troops occupied the town. His father was a shoemaker but died when the boy was small. His mother was a woman of great energy and with broad ambitions for her family of three boys and two girls. Frau Müller carried on the shoe business with some success; the soldiers who over-ran the place made a market for shoes. Johannes' family were staunch Catholics and the boy's early schooling was with the Jesuits; and it was his family's intention that he should study for the priesthood.

Now some authors who have written of Johannes Müller make much of his early education in its influence upon his later capabilities. In fact, his training has been used as an example of the need for a broader cultural education for modern pre-medical students—a feature upon which I shall comment more fully in a moment. Johannes as a youth received first a thorough grounding in Greek and Latin. It is said, probably with truth, although I find it said with almost monotonous regularity about the youth of famous scholars, that as a child he wrote Latin better than he did German and he found his recreation in making his own translations of Plato and Aristotle. He did remarkably well in mathematics; developed a hobby of collecting objects of natural history, except insects, toward which he had an aversion; and finally, he studied deeply in the humanities; he was a poet of some passing ability, an admirer of Shakespeare and Dante. In short, he had what today—except for the mathematics—we should call a typically cultural education. This fact, as I have said, has been used to impress a necessity for cultural studies in pre-medical education.

Thus Brücke went so far as to say that really worthwhile contributions to science have not come so much from those steeped in science

alone, as from those whose intellectual foundations have been deeper and wider. I do not for a moment doubt this fact, but I do doubt that the conclusions drawn from it can be, as is often attempted, applied to modern pre-medical education. Now it is remarkable but nevertheless a fact that medical educators may interpret matters of science with strict scrutiny and impartial judgment of cause and effect, but sometimes they seem to leave their scientific skepticism behind when they apply their judgment to other fields. With a wholly open mind in the matter I should not be convinced that a humanistic and classical education made Johannes Müller or anyone else a great contributor to medical science. I should wish to be shown that the relation was not wholly one of *post hoc*. Indeed Virchow said that Müller succeeded because he freed himself from the fetters of his early education. One may occasionally wish that the pre-medical education of today might have more science and the medical education less science and more art.

The basic element in the success of Müller is, I suspect, a tremendous intellectual endowment which allowed him to acquire the classics with ease, to read Latin at seven and Greek at ten and not the reading of Latin and Greek at seven and ten that makes his great intellect. We have few Johannes Müllers entering medical school today and there is always danger in attempts at emulation unless the emulator has the intellectual capacity of the man whose educational system he attempts to emulate. The physicians of the time of Molière were highly educated in the classics and in the humanities but that advantage, if it were an advantage, did not compensate for, rather it exaggerated, the deficiencies of a poor medical education. In this digression I merely stated my skepticism as to whether the classical and humanistic education of Johannes Müller was a determining influence toward his later medical productivity. There were many other physicians in Germany educated as well as he was and they did not reform German medicine; they merely devoted their efforts and their education toward the development of humanistically tenable but wholly unscientific systems. I believe that Johannes Müller was a man of superlative intellect and of a personality especially suited to the line of his career. And finally, I think he had opportunity and also some of that nebulous element called luck. Johannes Müller did not spring from the classics fully armed with the medical genius he displayed in later years. He had, as you will see, his difficulties to overcome and his adjustments to make and he did not always make them easily.

He was, as I have said, destined for the priesthood, but at about the age of sixteen he read with great enthusiasm the scientific writings of Goethe. The suggestion there was away from abstract thought and toward concrete factual nature; it was away from natural philosophy in which he had been steeped and toward the natural science of which he then knew so little. I have said that opportunity as well as intellect and personality are necessary for the display of greatness. Without Goethe's influence, Germany might have had a good priest in Johannes Müller and medicine might have lost one.

But whatever the forces were that changed the channel of his career we find him, at the age of eighteen, enrolled as a medical student at Bonn. There he was exposed to two great but opposing forces; one was the philosophy of Schelling toward which he was drawn both by his early education and by a strain of mysticism deep in his own make-up. It was this indefinable spiritual quality which I call here a strain of mysticism that perhaps later inspired his pupils and bound them to him as Virchow says in close ties as if by a religious bond. The other influence, the opposing force that drew him away from philosophy, from theorization and dreamy speculation was, strange to say, anatomy. Anatomy was a factual, realistic subject, the very antithesis of natural philosophy. In the struggle between these two forces, anatomy won out; it aroused his youthful naiveté to the extravagant exclamation that indicates his capitulation: "What does not come under the knife counts for nothing!" It was the extension of this view into the medical education of the nineteenth and early twentieth centuries in this country that made so painful for most of us here the first year of medical school.

Müller in his fourth year of medical education—1823—wrote a prize essay on the respiration of the fetus. Later Virchow, commenting on it, says that it was a work remarkable for the extent of the knowledge shown and for the ingenuity of the experiments carried out.

Having obtained his degree at Bonn, he went to Berlin to take his state examinations and there met, and for a short time worked with the physiologist, Rudolphi. Rudolphi held natural philosophy in contempt; he struck a responsive note in young Müller with his statement that anatomy was the foundation of medicine. Johannes, then twenty-two, was deeply influenced by the skepticism, the worldliness of this older man, and Rudolphi in turn, recognizing the merits of the youth—recognizing too, and perhaps pleased at his own influence upon the keen if still

naive mind, gave the boy an English microscope. The microscope was to play a determining factor in Müller's subsequent career.

The need to earn a living took young Dr. Müller back to Bonn where he made a rather meager livelihood from teaching and from a small practice; occasionally he was assisted by his mother. The death from peritonitis of a friend under his medical care convinced him that the practice of medicine was not to his liking; he dropped it and spent his time on his studies and teaching. These years, one may judge, were trying ones for Müller, a period of adjustments which he did not make easily. In the midst of it, at the age of twenty-five, he married Anna Zeiler, daughter of a landowner near Bonn.

In a poem which he wrote to her he promised her an immortal name in lieu of more material dowry. And then in the frantic burst of scientific research to gain that immortality, his health gave way. I do not know what his trouble was; a breakdown of a nervous nature his commentators say, and one cannot help but assume that these were days of frustration and perhaps unhappiness for a brilliant intellect, a driving ambition—a man torn between mysticism and the study of anatomy, newly married, poor, discontented with medical practice and without the scientific recognition he craved. And all this, be it noted, in an environment where the premium for intellectual endeavor was put on speculative flight of fancy. He did not break seriously under the strain; his health recovered and he returned to his researches. It was in this period that he did his work on the embryology of the generative system remembered in the duct of Müller; it was then also that he carried out his investigations on the nervous system and the sense organs and published his comparative physiology of vision. In that book he confirmed and established Bell's doctrine of spinal nerve fibers. All this and more were completed before he was thirty-two years old.

It was then that Rudolphi died and Müller was called to take his place in the chair of anatomy, physiology, and pathology at Berlin. The following year Müller published the first volume of his Manual of Human Physiology which was, as I have said, to exercise a determining influence in turning the German medical mind away from natural philosophy and toward science. It was to bring brilliant pupils to Müller's laboratory at Berlin.

It is of Müller as a pathologist that I speak particularly tonight. And in that field his important contribution to his students was his insistence

upon the use of the microscope in pathological study. This was a procedure virtually new in pathology and certainly unique as a routine. It was method that he urged upon his students, method of approaching problems and method of solving problems. Those methods must have become almost a primary way of thinking in the men who passed through his hands. They thought in the manner of quantitative evaluations sometimes in little matters as well as large. Thus when DuBois-Reymond wished to tell of the industry of his teacher, what was more natural than that he should use not adjectives but figures. He computed Müller's average literary output for thirty-seven years as amounting to thirty-five printed pages and 0.83 published plates drawn by his own hand each seven weeks.

I have mentioned the names of some of the more important students who were drawn to Müller and inspired by him. Best known of course was the aggressive Virchow, but for the discovery of Müller with which I deal tonight the most important was Theodore Schwann. It was Müller's insistence upon the use of the microscope that led Schwann in Müller's laboratory to discover the animal cell and postulate the cellular theory of tissue structure. It was this discovery which was not published in full by Schwann until 1839 that opened the way for Müller's discovery that cancerous growths were cellular; it also laid out the careers for two of Müller's most promising pupils: Henle, the histologist and anatomist, and, of course, Virchow.

So basic is Schwann's postulation to Müller's discovery that I digress for a moment to trace the outline of its development.

The original conception of a cellular structure is entirely from the botanists. In the seventeenth century Robert Hooke, using a magnifying glass, had noticed the "small boxes or bladders of air in cork." A more detailed structure of plant cells could not be investigated until the compound microscope was developed. It was 1833, the same year that Müller went to Berlin, that the botanist Robert Brown discovered the nucleus in the plant cell. In 1836 (although published in 1838) Schleiden proved that plant tissue is made up of cells and developed only from the multiplication of cells. Müller's amiable and phlegmatic pupil Schwann had seen nucleated cells in animal tissue. Influenced by Schleiden's work he searched for cells in all the tissues he knew of. He found them and from his findings formulated the basic law of morphology for all vegetable and animal tissue. To quote his words: "There is one universal principle of

development for the elementary parts of organisms, however different and that principle is the formation of the cells."

One can almost sense the excitement that must have pervaded the laboratory of Johannes Müller in those years of 1836, 1837 and 1838. Remember that this was before Virchow came there as a student; he was to make the greatest advances with cellular pathology and to obtain the greatest recognition but the fundamental discoveries were made before he came to the laboratory in 1839.

I have already read from the works of Müller how he was led to turn his microscope on cancerous growths and to see the cancer cell and see further that it took the general shape of the cells of tissue from which it sprang.

For my purposes here this basic discovery made public in the year 1838 is as far as I wish to go with the work of Müller. He died, as you know, at the age of fifty-seven, probably of an arteriosclerotic accident, for he was found dead in the bed to which he had retired in good health.

There is just one more quotation from Müller that I make before I leave him. Müller was not infallible but he was sometimes dogmatic. Thus on one occasion he said that the rate of transmission of the impulse in the nerve fiber would never be measured; within a decade his pupil Helmholtz had measured the rate. And again—and this is the quotation pertinent here—when he had finished his work on the histological classification of cancers he wrote in his book these words:

"Microscope and chemical analysis can never become a means of surgical diagnosis for malignant growths; it were ridiculous to desire it, or to suppose it practicable."

Now if there is any one thing in which microscopic examination of neoplasms has been useful, practicable, it is diagnosis.

The enormous volumes of microscopic anatomy of neoplasms are at once a refutation and also a little justification of Müller's dogmatization. The morphological studies for classification go on endlessly with a descriptive refinement that forces one to the conclusion that while the microscope shows the cancer cell, this cell itself has no fixed morphology, only approximate. If one may hazard here a dogmatization with all dangers of dogmatization it is that cancer morphology has not and will not contribute toward the really fundamental discovery sought today—the reason for the cancer cell—cancer causation. The search today still centers on the cell but not its shape, rather its physiology, not cellular

morphology, but cellular reaction.

Johannes Müller's statement that cancer is cellular has remained since his day the foundation of all cancer research. Literally everything we know about cancer, except its gross appearance, has been gained in the single century that has passed since his publication of 1838.

Cancer causation was from the beginning and is today the great riddle. And such knowledge as we have gained and are gaining makes each year more dubious the possibility of any completely successful method of treating the developed cancer. I do not mean in any way to belittle the achievements of the therapeutists or to imply that the wider and fuller application of their measures would not save many lives. It would. But everyone in medicine knows that the often quoted slogan of early detection and cure has elements of well intended sophistry. They know, too, that no new successful principle of treatment has been developed since the time of Hippocrates. In his day there were surgery and caustic plasters; in our day there is better surgery while radium and x-ray have replaced the caustic. The aim then was, and the aim now is, to remove or destroy the neoplasm. The principle is unchanged. We all see, I think, that success must lie in other directions. And the essential to that success must be the discovery of cancer causation. Occasionally specific therapy is discovered accidentally before the cause of the disease is known. But such is rare. Usually the discovery of the cause must precede the development of treatment or prevention.

The scientists of Müller's day saw that fact and they gave us hypothesis; there was Virchow's chronic irritation theory, Cohnheim's stimulation of misplaced embryonic rests with Ribbert's later modification; there was the heredity and there was the parasitic theory.

The experimental scientific investigations that have given promise of the eventual solution of cancer causation showed first the autonomy of the cancer cell. Hanau, as early as 1889, made successful transplants of cancer cells in rats; his work was confirmed in 1901 by Loeb and in 1903 by Jensen. It was carried to its logical conclusion by the *in vitro* growth of cancer cells by the method of Harrison.

Next in these few and broad steps I take in reviewing cancer study was the experimental production of cancer. This new era in cancer research started with Fibiger's discovery of the cancer in the stomach of the rat caused by a nematode carried by the cockroach—in itself one of the most exciting stories in modern scientific research. Fibiger's work

stimulated interest and efforts to produce experimental cancers and next came the tar cancers in the ears of rabbits.

It is a long jump from there to the discovery in tar of the carcinogenic penathrene ring; and the discovery of this ring in normal secretions of the body and in vitamins. It is a long jump too from simple tar cancer to the virus cancer of Rous and still another to the isolation of at least one virus as a non-living chemical substance. Today cancer research moves from the microscope to the test tube; from morphology to chemistry. And the promise held today for the discovery of cancer causation and for the subsequent relief from the scourge of cancer grows brighter.

The great danger to the search for the solution of the cancer problem is not ignorance; not lack of means of approach; not failure of science; it threatens always from another direction. It is the danger that some social cataclysm shall shake the rock of science and dislodge the scientist. It is then that men's minds lose the desire for facts, for realities; they turn to the consolation of philosophies, they turn to bizarre social manifestations. This has happened in the past; it may well happen again. Science, as we know it, is a solid structure but the scientist who builds upon it is a man and as a man is at the mercy of his social environment. That is the frail structure and the scientist is no stronger than its strength. German thought of a century ago rose as I have traced from speculation to productive science; within our time it fell again. These things are not of one country, or of one race, or of one century. They are universal and eternal. And so I say again, barring the social cataclysm that will dislodge the scientist, the way is clear and open to the discovery of cancer causation.

Perhaps in this building or its successor a hundred years from tonight there may be again, with the problem of cancer then solved, the suggestion that the memory of Johannes Müller, who defined the problem for its solution, be revered. You and I will not be here. But I hope that he who is privileged to recall the memory of that occasion will recall the man as he is recalled in the words of his pupil Virchow. They are almost a prayer in the rise of science above social retardation. He said:

"The cult which he served as a priest of nature bound his pupils to him in close ties, as if by a religious bond; and the serious priestly fashion of his speech and movements compelled the veneration with which everyone regarded him. His mouth, with its tightly compressed lips, conveyed a notion of severity; around eyes and forehead played an expres-

sion of profound thought; every furrow in his face stimulated the idea of a perfectly finished work—thus did the man stand before the altar of nature, freed by his own energies from the fetters of education and traditions, a living witness to personal independence!"